rhetoric about covering-law models, hypothetical-deductive methodology, etc., the point seems to have been missed as to the substance of these constructs: namely, that in scientific explanation *à la* Hempel and Oppenheim arguments are constructed following the canons of deductive logic and relate observation statements *deductively* to covering laws and antecedent conditions. In a deductive explanatory argument, the justification for an assertion that a statement *P* follows from a statement *Q* is neither reasonableness nor plausibility, but logical necessity. If, however, all that is meant by "explanation" is the commonsense notion, then the so-called epistemological revolution is no such thing. Surely no archaeologist has ever denied that arguments must be reasonable.

Given the failure to establish, for the most part, the deductive rigor which is being held out as a model (there are notable exceptions—Zubrow [1975] attempts to provide the deductive arguments but fails admirably), we are left with the uneasy feeling that what is being said (explanatory arguments) and what is being done are quite different. In fact, though there seems to have been a redefinition of the name of the game (calling it explanation, but using the day-to-day logic of plausibility of the "old archaeologists"), the significant shift has been in the meaning to be attributed to archaeological data (archaeological data as the "frozen" representation of a past cultural system [cf. Binford 1962]). It is change in expectable meaning, and not explanation, that has had the most profound impact on archaeological research, as a perusal of research articles in the major archaeology journals shows. These articles are not necessarily any more rigorous in their use of deductive argumentation than earlier ones, but they differ radically in their ideas as to what kinds of arguments are valid using archaeological data as a basis. The revolution is in the meaning of archaeological data; "being scientific" has been the metaphorical expression of that revolution. But meaning is not automatic. It is a consequence, as Binford (1977) among others has argued, of the theoretical framework into which the bits of data are inserted.

We can now begin to see more clearly the reason for covering laws; the framework being constructed is not about the material detritus found by the archaeologist per se, but about cultural systems. The goal is to understand the material cultural system through the ideational one of which the material system is an empirical expression. But what is the linkage between the detritus of the field archaeologist and the explanatory arguments of the archaeological theoretician? It must be a series of propositions, either themselves lawlike or the consequence of lawlike statements formulated at a higher level of abstraction. But how do we choose amongst the potentially thousands of propositions that can be formulated for ones to develop? It is futile to search randomly; the utility of a proposition is dictated by its relevance to a body of theory. The detritus found by the archaeologist is the consequence of physical processes operating on the material remains of a past culture, which in turn are the consequence of behavior given form by a conceptual framework which is the reason that particular set of animal behaviors can be called "human." The laws for which the archaeologist searches are laws of conceptual systems that incorporate the biological givens of human abilities and are interfaced with physical laws and properties which constrain, limit, and define the range of their material expression.

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# More on Neanderthal vs. *Paranthropus*


*by* GORDON STRASENBURGH

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I take up the burden of proof graciously urged upon me by Bayanov and Bourtsev (CA 17:312–18) with a great deal of pleasure. I regret that the brevity of my letter (CA 16:486–87) was misunderstood. There is a viable alternative to Porshnev's (CA 15:449–56) hypothesis, and I prefer it. So that there will be no further misunderstanding, I want to dedicate these remarks to Porshnev, a tenacious and brilliant man. Call us hominologists, sasquatch-lovers, or simply interested parties, Porshnev was first among us. If he would not have agreed with the substance of the *Paranthropus* hypothesis, I feel certain he would have understood my intent in presenting it.

Chronology is central to my argument. Porshnev and his contemporaries spoke guardedly of 1,000,000 years in a murky past. Now geologists tell us that Africa and South America drifted apart 100,000,000 years ago (Dietz and Holden 1970). If we use that date, rather than 35,000,000 years, for the separation of the Old and the New World monkeys, then the protein serum-albumin method of Sarich and Wilson places the "missing link" at about 15,000,000 years. Some paleontologists place it in that very range by another logical route (Edey 1972).

We have a view from the hominid fossil evidence of the latter third of that period. It has become increasingly clear over the




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past two decades that for four-fifths of that period there were two hominids, generally distinguished as "robust" and "gracile."

The rather tentative synthesis I see beyond, for our side of the family, is as follows: There were three chronologically contiguous species of *Homo: (Australopithecus) africanus, erectus,* and *sapiens*. Our genus has got progressively bigger: *H. africanus*, brain 500 cc, weight 50 lb. (23 kg), height 4 to perhaps 4½ ft. (1.2 to 1.4 m); *H. erectus*, brain 1,000 cc, weight 100 lb. (45 kg), height 5 ft. (1.5 m); *H. sapiens* (to include Neanderthal), brain 1,500 cc, weight 150 lb. (70 kg), height 5½ ft. (1.7 m). Our hunting tools have become progressively more sophisticated, and in recent times we have expanded into every habitat suitable for the quarry of a social carnivore turned farmer.

The picture on the other side of the family is less clear. *Paranthropus*, it seems, was both taller and more robustly built than contemporary *Homo* (Robinson 1956). His brain was slightly larger (Holloway 1974). Robinson (1972) makes him out to have been a woods-dwelling herbivore, or graminivore, after Jolly (1970). Beyond this, we know only that the genus survived for 4,000,000 years with little apparent change. This could be taken as an indication that *Paranthropus* was a superbly generalized animal. In support of this view is the range of *Paranthropus* from Java to South Africa.

Those who recognize *Paranthropus*, or simply two australopithecines of somewhat different adaptation and size, generally consider the larger form to have become extinct about 500,000 to 1,000,000 years ago. This hypothetical extinction raises more questions than it answers. What was the agency? *H. erectus* has been suggested, but there is no evidence to support that hypothesis. Nor does it appear likely that a species which devoted some of its time to headhunting could simultaneously have undertaken the elimination of *Paranthropus*. The absence of *Paranthropus* fossils in the recent past may simply reflect the fact that anthropologists are *not yet looking* for the fossils of woods-dwelling hominid during the period in question, coupled with a dramatic drop in all known fossils during the first half of that period.

In a 15,000,000-year context, Neanderthal is relatively recent. While there is much we do not know about Neanderthal, everything we do know suggests that he was the man of his time and region in cultural, behavioral, and physiological terms. *Paranthropus* is many times more ancient. Again, there is much we do not know about *Paranthropus*. What we do know suggests that *Paranthropus* bears a far greater noncultural, behavioral, geographical, and physiological resemblance to wildman than does Neanderthal.

Wildman is a species, real or imaginary, which is (1) larger and more powerful, or larger, or more powerful, than man. Wildman is (2) a hominid (3) covered with hair, and (4) a sexual distinction can be made among adults. Behaviorally, wildman is (5) aquatic, (6) nocturnal, (7) solitary, and (8) a woods and mountain dweller, with (9) vocalizations in the range of whistle/scream. Wildman is often regarded among traditional societies as (10) Lord of the Animals. That descriptions of wildman from the northern hemisphere, past and present, are remarkably consistent is illustrated by Green (1978).

A second, somewhat more complex, dimension to the *Paranthropus* hypothesis requires a more selective and detailed review of the data. At the suggestion of my Soviet colleagues, I gladly accept Europe as the major theater of debate.

That wildman inhabited Europe during the first half of the millennium now closing is beyond dispute. On the question of myth versus reality, Porshnev, Bourtsev, Bayanov, and I are of like mind; we are compelled by the results of our method of approach to the question. I cannot improve on the Russians' framing of that method: "the comparative analysis of mutually independent evidence." Soviet and North American students of the question have only recently begun to exchange data, observations, and conclusions. In doing so, we find ourselves, with the exception of the question of ancestor, in substantial agreement.

At the opening of this millennium, Europe, as best we can tell, was a great forest with wide expanses of marsh and swamp in the north. Its transformation began with an agricultural revolution of nearly three centuries' duration. Europeans cut down the forest; brought the cleared land under cultivation; built towns and cities, warmed themselves, and cooked with the timber. Shortages of wood for fuel are reported as early as the third quarter of the 16th century in some regions (Krantzberg and Pursell 1967). But for the discovery and exploitation of coal as an alternative fuel source, there would have been no Industrial Revolution. By the mid-17th century, the population of Europe had grown to 100,000,000.

European man systematically destroyed the habitat of wildman as he constructed the agricultural and demographic foundation of European civilization. The gentry, mounted, armed, and armored, went questing after wildmen. Their fantastic regalia may well have been designed to attract wildmen, and the camouflage of their mounts may have been intended to afford them psychological advantage in the ensuing combat. A far greater blow to the wildman population than all the knights in Christendom, however, would have been the plague. The plague first struck in 1347, with recurrences through 1400. The western European population fell by roughly half and is thought to have been a century in returning to its pre-plague level. What we know about pongid susceptibility to human disease suggests that the wildman population would have fallen prey to the ravages of the Black Death to no less an extent than did the human one. While Bernheimer (1970:3) makes clear his predisposition that the "notion of the wildman must respond and be due to a persistent psychological urge," he dutifully reports (p. 71): "After the years of the Black Death there is in England a silence on the subject which lasts throughout the time of the Wars of the Roses."

Against this background, we may now examine the two latest and most authoritative descriptions of European wildmen, cited by Linnaeus in erecting the species *Homo sylvestris*, or *Homo nocturnus*, or *Homo troglodytes*. The penultimate report comes to us by the hand of Nicolas Tulp, noted city father of Amsterdam and pioneer surgeon. It appears in the second, 1652, and subsequent editions of his *Medical Observations* (see Tulp 1672). Tulp's Latin is troublesome, but with one exception his description is thrice replicated in translation. The exception is *rudis*, a word of as many shades as its English approximations "rough" or "wild." The word, according to Bernheimer (1970), is so closely associated with the wildman in Europe as to be almost an epithet. In a note to a 12th-century German source, he makes it "hairy." For Tulp's descriptive string "fronte ut obtusa, ac depressa, sic occipitio convexo, ac tuberoso, rudis, temerarius, imperterritus," Bendyshe (Blumenbach 1865 [1811]) offers a 1716 translation "with retreating and depressed forehead, but convex and knotty occiput, rude, rash, ignorant of fear." Zingg and Singh (1939) have "a low retreating forehead domed up to a bumpy occiput. His actions were crude, fearless, and unplanned." In an informal translation done for me in 1972 we find "a face, which as it was dull and flattened, so his occiput was convex and ridged; he was wild, hasty, unfrightened." Is Tulp then silent on the question of hair? There is one further descriptive sentence, rendered in 1716 as "His appearance was more that of a wild beast than a man" and in 1972 as "He bore the appearance more of a beast than of a man." Tulp describes his eating behavior in some detail: he refused ordinary fare and ate only grass and hay, choosing that carefully as a sheep might. Tulp concludes the "16-year-old youth" is a feral human brought up by sheep and much influenced by their appearance and manners.

What exactly is Tulp describing? We may be reasonably certain it is not an ape, for in the first (1641) and subsequent editions of *Medical Observations* he describes (judging from the



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text, the plate which accompanies it, and a reputed origin in Angola) a chimpanzee. It was hairy ("at pone [*hirsutus*]"). The plate is entitled "man of the woods" in two languages: "*Homo sylvestris*, Orangutan."

The last European wildman cited by Linnaeus is Wild Peter. Blumenbach (1865 [1811]:183) describes him as "a naked, brownish black haired creature" the size of a twelve-year-old boy and reports (p. 184): "When anything was offered him to eat, he first smelt it and then either put it in his mouth, or laid it aside with a shake of his head." He liked "green beans, peas, turnips, mulberries, fruit, and particularly onions and hazelnuts." Blumenbach concludes that Peter was an idiot. Wild Peter was the pet of the English kings George from 1726 until at least 1767, when two London papers (January 8) carried the following news item (quoted in the *Virginia Gazette*, April 30, 1767):

> Yesterday the wildman, well known by the name of Peter, that was taken in a wood in Hanover, about forty years ago, and brought over to England, since which he has been kept at Cheshunt in Hertfordshire, supported by an annual stipend allowed, was brought from thence to the Queen's palace, to be seen by the Royal Family; he is supposed now to be upwards of sixty [*sic*] year of age, cannot speak anything intelligible, and still retains his wild appearance.

Two wildmen are reported taken in North America, one alive and one dead. Jacko, the former, was captured near Yale, British Columbia, in 1884. The news account (cited in Green 1978) describes him as "something of the gorilla type, standing about four feet seven inches and weighing 127 pounds. He has long, black, strong hair and resembles a human being with one exception, his entire body, excepting his hands (or paws) and feet are covered with glossy black hair about one inch long. His favorite food so far is berries, and he drinks milk with evident relish." The latter, popularly known as the Minnesota Iceman, has been reported on at length by Napier (1972), Heuvelmans and Porshnev (1974), and Sanderson (1969). Of special interest are the high, prominent cheekbones which Sanderson included in his rendering of the specimen as it lay and then omitted in redrawing the body without hair.

The great majority of wildman reports in North America arise from fleeting encounters with solitary adults. Facial and cranial characteristics and comments on diet are therefore rare. Two descriptions collected by Green (1978) in the mid-'50s contain the following information: The late Albert Ostman swore that he had been carried off in his sleeping bag during the night by an adult male in 1924, after which he had spent several days with a family of four in the mountains of British Columbia. In 1957, he remembered the adolescent male as having "wide jaws, narrow forehead, that slanted upward round at the back four or five inches higher than the forehead." Of their eating habits he reported: "They might eat meat, but I never saw them eat meat, or do any cooking. I think this [box canyon] was a stopover place and the plants with sweet roots on the mountain side might have been in season this time of year. They seem to be most interested in them. The roots have a very sweet and satisfying taste." The late William Roe, who observed an adult female in the mountains of British Columbia in 1955, swore in 1957: "The head was higher at the back than at the front. The nose was broad and flat. The lips and chin protruded more than its nose." Roe states that after the encounter "I found spore in five different places, and although I examined it thoroughly, could find no hair or shells of bugs or insects. So I believe it was strictly a vegetarian." Dahinden (Hunter and Dahinden 1973) collected the report of Mrs. Calhoun after her encounter with an individual in British Columbia in 1962. "It had high cheekbones, a wide, flat nose, a forehead that sloped back, and a mouth that stuck out." The adult wildman with breasts in the Patterson-Gimlin film taken in northern California in 1967 has been repeatedly described by primatologists as crested, and a flat facial profile can be discerned.

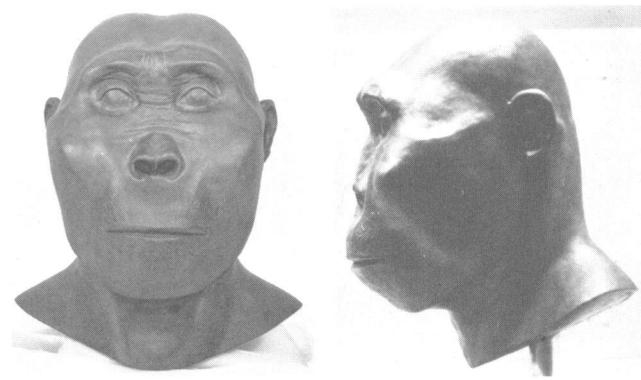

FIG. 1. Flesh restoration of a well-preserved, crested australopithecine skull found at Olduvai by Mary Leakey in 1959, commissioned by the National Geographic Society and executed by T. Dale Stewart and Jay Matternes (a Smithsonian Institution exhibit reproduced with that institution's permission; camera taking left photograph was well below Frankfort plane).

The facial and cranial features we have reviewed recur in the Soviet data as well (Bayanov and Bourtsev p. 317; Tchernine 1971). They are also found in, if they are not exemplified by, the fleshing by Stewart and Matternes of a paranthropine skull found by Leakey (figure 1). Further correspondence is to be found between the North American and the Eurasian data in the matter of behavioral characteristics. Of special interest is the aquatic capability of wildman. I thoroughly agree with Bayanov and Bourtsev when they speak of the aquatic capability of the hominid family. I would suggest two components for the adaptation: physiological and psychological. In North America we are learning that, given exposure to deep water as an infant, *H. sapiens* exhibit a remarkable adaptation to it. In terms of physiological adaptation to cold water, however, adult females are significantly superior to males. The difference is subcutaneous fat. One of Green's witnesses comments on the lack of muscular definition in an animal so obviously powerful.

Adult wildpeople in North America stand 7 to over 9 ft. (2 to 3 m) and weigh 500 to 1,000 lb. (from over 200 to over 400 kg)—"Lord of the Animals" indeed. If wildman is the descendant of *Paranthropus*, analogous to the paranthropine subfamily, or Neanderthal, then the brain is, in terms of volume and of configuration versus that of the pongids, presumably equivalent to ours. Adolescent and preadolescent males predominate among reportedly taken wildmen beginning with Enkidu, the companion of Gilgamesh, the account of whose capture was written down 3,500 years ago (Sanders 1972).

None of these three observations bear on the validity either of the *Paranthropus* or of the Neanderthal hypothesis. Rather, they bear on the validity of the question both hypotheses seek to answer: What is wildman? I am convinced that if anthropologists will seriously address this question, *Paranthropus* will emerge as a plausible and instructive alternative to Neanderthal.

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